Atmos. Chem. Phys. Discuss., 9, C7095–C7100, 2009
www.atmos-chem-phys-discuss.net/9/C7095/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Numerical simulations of contrail-to-cirrus transition – Part 1: An extensive parametric study" *by* S. Unterstrasser and K. Gierens

Anonymous Referee #3

Received and published: 16 November 2009

General Comments

This paper describes a set of two-dimensional computational simulations of contrails up to 6 hours after their emission. The results from a wide range of simulation conditions are presented with detailed analysis that focuses on characteristics of the results that are relevant to investigating climate impacts. Compared to previous contrail simulation studies, this paper presents a plethora of useful data, and I would rate its scientific significance as excellent.

I have some concerns about the application of a two-dimensional model to what is clearly a three-dimensional problem. The authors have attempted to address these

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by way of a validation example, but I think that additional acknowledgement of the limitations of a two-dimensional simulation is called for. The validation comparisons to contrail observations are interesting and provide some more confidence in the results. I would rate the paper's scientific quality as good.

I would rate the presentation quality of this paper as fair. I appreciate the difficulty of presenting the large amount of data produced by the parametric study, but unfortunately I found the plots presenting 16 lines at once to be somewhat confusing and difficult to interpret. The overall writing style was occasionally informal and not always correct English, with frequent misuse of prepositions and some awkward sentences.

I recommend that this paper be published with substantial editing for style and grammar. If the authors can find a way to clarify the line plots, I would recommend that revision as well. I have detailed some scientific questions and technical corrections below.

Scientific Questions

I would like to see a few more references in the introductory material. For example, p. 14903, lines 11-14, could reference the study that is referred to (perhaps Minnis et al. 2004). Section 1.2, paragraph 1, could mention more previous LES contrail work (Lewellen and Lewellen 2001, Chlond 1998, Shirgaonkar and Lele 2007). It might be helpful to refer to the differences between this previous work and the present paper.

p. 14904, line 8-9: The proper definition for the end of the jet phase should be when the characteristic velocity of the jet has decreased to the same order as the velocity induced by the wing vortices.

p. 14905, line 7: What is the "CC-tool"? I believe this references the vortex decay imposed in UGS08 to simulate 3D effects on vortex decay, but it might be best to not mention the name given in the previous work here.

Section 2.1, paragraph 1: I'm curious as to the assumptions that are made in this model

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to approximate a turbulent 3D flow in 2D. In the abstract, the simulations presented here are called ”LES”, but no mention is made of a sub grid scale turbulence model or how the model accounts for 3D effects. The paper cited for EULAG does not present a 2D model, but rather a 3D model. Since the beginning of the simulations presented here still contain vortices (p. 14906, line 10-12), and their breakup and dissipation is a 3D process, I wonder whether the initial condition used here is really appropriate for a 2D simulation. The current model compares favorably to a 3D LES in terms of plume diffusion (section 4.1), but the 3D LES there did not include any vortex system in its initial condition.

I do not believe that neglecting 3D effects has made a large difference in the results presented here, but I would still like to see more discussion of this and some acknowledgement that a level of physical fidelity has been sacrificed to allow more simulations to be undertaken.

p. 14906, line 19-26: The grid resolution has presumably been set to balance computational cost against scale resolution, and some discussion of this selection is warranted. At the very least, a statement that a grid refinement study was conducted and showed little sensitivity at the selected resolution should be included.

p. 14906, line 12-16: Along the same lines as above, the authors could describe the ”suitable modification” to the vortex velocity field in more detail. This may be how 3D effects are taken into account for the initial field, but the process is not clear.

p. 14914, line 17-18: The statement that ”the simulations show that vertical turbulent diffusion is too weak to expand the height of the core region over time” is worrisome, since the validation results in section 4.1 show that the 2D model presented here underestimates vertical diffusion compared to a 3D model. Again, I think the impact on results is probably small, but this is an example of why results from a 2D model must be interpreted with caution.

p. 14914, line 25-26: I don’t see evidence in Figure 3 that the contrail becomes weaker

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after ”some hours”. The peak optical depth shown here has increased from 2000 s to 6500 s, and then decreased out to 11000 s. The peak at 11000 s is approximately the same as it was at 2000 s, and is certainly still distinguishable as a peak (not ”less distinguishable from the fallstreak”). Instead, I would say that the optical depth of the fallstreak has increased. Also, to say that the core region has become ”dehydrated”, I would like to see some evidence that the water content in the core has actually decreased ”for example, by showing a plot similar to Figure 3, but instead integrating IWC horizontally.

p. 14917, line 14-18: The authors claim that ”without sedimentation, the IWC in the contrail core would be conserved because the ice mass and the area would increase with the same rate”. I don’t see how this follows from the argument presented, and without a plot of contrail area versus time, it is difficult to tell whether this might be true.

Section 3.3.4, paragraph 1: The authors’ analysis does not account for large peaks in the IWC shown in Figure 6 (right) around 5000 s for several of the simulations (dotted lines).

p. 14920, line 16-17: A vertical profile is referred to here that does not seem to exist in the figures.

p. 14920, line 23-24: Is there a reference which describes why this derivation of effective radius is the closest to satellite measurements?

p. 14924, line 4: The authors state that the rms-value of turbulent fluctuations becomes quasi-constant over time. Is the turbulence being driven to sustain it throughout the simulation? This should at least be mentioned if not described in detail.

Figure 11: Why does the plot of the skewed variance stop at 3000 s?

Technical corrections

In the abstract and throughout the paper, the abbreviations RH*_i and RH_i are used interchangeably and should be made consistent. The same goes for T and T_CA.

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Interactive Comment

The definition of relative humidity used throughout the paper is actually relative humidity with respect to ice. This should be mentioned at least once.

p. 14902, line 3: change ”properties on” to ”properties to”

p. 14904, line 9: change ”emerged” to ”merge” (although this statement is not correct as noted above)

p. 14905, line 11: capitalize Lagrangian

p. 14906, line 5: period after Sect. 4.1

p. 14906, line 19-21: correct ”However it is not worth to separate”

p. 14908, line 4: change ”less” to ”fewer”

p. 14909, line 7: change ”esp.” to ”especially”

p. 14910, line 5: change ”sensitivity on” to ”sensitivity to’

p. 14910, line 19: change ”sensitivity on” to ”sensitivity to”

p. 14912, line 3: change ”a spreading” to ”spreading”

p. 14912, line 20: change ”to naturally formed cirrus as” to ”from naturally formed cirrus where”

p. 14913, line 1: change ”special of” to ”unique to”

p. 14918, line 22: correct ”allows to compare the results”

p. 14918, line 25: correct ”it follows a sharp increase within the next minutes”

p. 14921, line 4: change ”sensitivity on” to ”sensitivity to”

p. 14921, line 10: sudden use of ”b” rather than ”right” or ”left” to refer to subfigure

p. 14921, line 3-10: this paragraph is not very clear define the ”effects” that are referred to

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p. 14926, line 17: correct ”Contrary we use an absolute threshold”

Section 5: The discussion section is, in general, very informal in style and needs revision.

p. 14931, line 25: change ”less” to ”fewer”

Add section numbers for easier reference: p. 14914 line 7, p. 14916 line 23, p. 14918 line 10, p. 14918 line 15, p. 14926 line 8 (table number)

Table 3: I found this table to be very confusing and difficult to understand. It should probably be reformatted and clarified.

References:

Minnis, P., J. Ayers, R. Palikonda, and D. Phan, Contrails, Cirrus Trends, and Climate, J. of Climate, Vol. 17, 1671-1685, 2004.

Lewellen, D.C. and W.S. Lewellen, The effects of aircraft wake dynamics on contrail development, J. of Atmos. Sci., Vol 58, 390-406, 2001.

Chlond, A., Large Eddy Simulation of Contrails, J. Atmos. Sci., Vol. 55, 796-819, 1998.

Shirgaonkar, A.A. and S.K. Lele, Large Eddy Simulation of Early Stage Aircraft Contrails, Flow Physics and Computational Engineering Group, Department of Mechanical Engineering, Stanford University, No. TF-100, 2007. Available online: http://www.stanford.edu/group/fpc/Publications/TF/TF-100.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14901, 2009.